November 25, 1953

Dr. Francis J. Ryan
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Dear Francis:

Your post card dated Rome, September 8, arrived here not very long ago and Esther and I want to say how very much we did appreciate the good feeling that prompted you and the others to think of sending it. Because Ephrussi wrote me quite flatly that there had been some sort of rumor as to possible reasons why we had not attended the meetings in Europe I hope I can confirm to you that this was purely and simply a financial matter, though I must admit our reserves of energy as well as finances were rather low. We did manage, however, to make a trip to California to see Tatum and VanNiel as well as to attend to other matters. I am sure that you and Betty enjoyed this year's trip to Europe with your usual gusto. I am only sorry not to have a better opportunity to hear about it from you at first hand.

I think that Franz Schrader would be rather amused to hear that I am turning back, at least for the present few months, to cytology. I have no hope of cleaning up the mess that now pertains to the status of the bacterial nucleus, but we do have some basis for optimism on the matter of detecting the cytological basis of mating, in strain K12. I will not take time now to give you the laboratory's full annual report, but I thought you might be interested that Tom is very busy in a genetic problem concerning the role of S polarity in determining the direction of elimination from the heterozygotes in K12. His present work strongly supports our earlier conclusions that these eliminations occur after there has been an opportunity for crossing over and not in connection with the formation of the gametes as Jim Watson and Bill Hayes had proposed. At the instant ~~that~~ I am myself particularly concerned with the isolation of the immediate products of mating in high frequency stocks. I have still to complete the necessary single cell isolations, but Ethyln Lively had done a few before in connection with another problem and these concur with our single colony isolations to affirm that the mating cells comprise the whole genetic content of both parents. However, the recombinants

from these same zygotes show the elimination behavior previously described. That is, we can recover from a single colony presumably derived from a single cell the two parents intact as well as a limited range of recombination types, but you will hear more of this when we have more thoroughly confirmed the experimental results.

I have just seen your very readable article in the Scientific American. I was flattered that you should have relied on the method of indirect selection as the most direct proof of the hypothesis of preadaptive mutation. I am, however, sorry that you over-looked to include Esther as the co-author of this work, as indeed she was. But, that is only a little water under a very unimportant bridge. If the publishers happen to have furnished you with a few tear sheets or reprints of this article, we would appreciate having one. You might be interested that it was called to our attention by my youngest brother, Bernard, who is living in Philadelphia now with my parents and not quite yet in high school.

Yours sincerely,

Joshua Lederberg

JL/gw